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THE RECENT DEVELOPMENT OF METHOD IN THEORETICAL PHYSICS.¹

IN former centuries, the progress of science, as exhibited in the labors of its most gifted representatives, though continuous in its movement, was relatively slow, like the growth of an ancient Continental city to which industrious and enterprising citizens have kept constantly adding new and improved structures. The present century, on the other hand, with its steam and its telegraph, has impressed the stamp of its own nervous and restless activity upon the progress of science; so that the development of the physical sciences in recent times resembles rather the growth of a modern American city which has sprung in a few decades from an insignificant village to a great metropolis.

Leibnitz has been correctly described as the last man who was able to compass the entire knowledge of his age. True, there have not been wanting in recent times men who have evoked astonishment by the prodigious extent of their knowledge. I have but to mention here the name of Helmholtz, who was master alike of four different provinces of knowledge,—philosophy, mathematics, physics, and physiology. Yet even these four sciences, broad as is their extent, were limited and more or less related provinces only, of the grand total of human knowledge, which is immeasurably greater.

The consequence of this stupendous and rapid augmentation

¹ From the proceedings of the seventy-first congress of the *Gesellschaft deutscher Naturforscher und Aerzte*, held at Munich, September 17–23, 1899. Translated from the German by Thomas J. McCormack.

of our positive knowledge has been an exceedingly minute division of labor in science, approaching in intricacy the systems of some of our great modern factories. Unquestionably such a division of labor is favorable in an eminent degree, nay, even indispensable, to the rapid development of science; but it is no less certain that it carries with it the possibility of grave dangers. There is wanting to it that broad view of the whole which is an indispensable requisite of ideal scientific research, the aim of which is the discovery of new points of view, or of new combinations of old points of view. In order to offset this drawback as far as possible, it is unquestionably of advantage if some individual who is engaged in such specialised scientific work endeavors from time to time to present to the scientific public at large a survey of the development of the special provinces of knowledge in which he is working.

But such an undertaking is attended with grave difficulties. The infinitely extended chain of inferential or experimental investigation which has some definite result for its ultimate goal, does not lend itself to ready and synoptic comprehension save for persons who have made the pursuit of such trains of thought the labor of a lifetime. Then again, for the purpose of abbreviating phraseology and facilitating breadth of view, numerous strange and learned words have been introduced into science. And while on the one hand it would be out of place to exhaust the patience of one's readers by explaining all these new ideas before coming to one's real subject, on the other hand intelligibility is impossible without some account of them. Moreover, popular exposition cannot in itself be regarded as the main object; to make it such would unfailingly result in the emasculation of that rigor of deductive reasoning and in the abandonment of that exactitude which has justly become the distinguishing attribute and the pride of physical science. In selecting, therefore, for my present theme a popular presentation of the modes of development of theoretical physics in recent times, I am thoroughly conscious of the fact that my purpose is not attainable in that perfection in which my imagination has pictured it, and that I shall be able to offer but a rough outline of the most essential points only; whilst I shall also necessarily

afford frequent occasion for complaint in presenting many things which are perfectly known to all, but which I am obliged to touch upon to render my expositions complete.

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The principal cause of the rapid development of physical science in recent times is unquestionably the discovery and perfection of suitable methods of investigation. In the experimental field, the methods are frequently quite automatic in their action, and the inquirer in a measure is merely obliged to furnish new material for his research, as a weaver supplies fresh thread for his loom. Thus, for example, the physicist merely investigates different new substances with respect to their tenacity, their electric resistance, etc., and then repeats his experimental work at the temperature of liquid hydrogen and again at that of Moissan's electric furnace. And the same statement holds true with regard to much of the work of chemistry. Of course, there is always a goodly measure of acumen still essential for determining the precise experimental conditions in which the investigations shall be successful.

The case is not so simple with the methods of theoretical physics; yet here too we may speak in a sense of automatic procedure.

The high importance attaching to correct method explains why men have reasoned not only concerning things, but also concerning the method of our reasoning concerning things. Thus arose the so-called theory of knowledge which, despite certain lingering traditions of the ancient and now tabooed metaphysics, is of the highest significance for science.

The development of scientific method is, so to speak, the skeleton which carries the development of the entire body of science. I shall, therefore, pay principal attention in the following pages to the development of methods, and shall make use of the actual results of science merely for the purpose of investigating these methods. The results are in their very nature better known and more easy to comprehend, whereas their methodological relationship is precisely what is in most need of investigation.

It is particularly fascinating to proceed from the retrospect of history to the outlook upon the scientific development of the future,

which is denied to us by the shortness of human life; but on this point I shall confess at the outset that I have only negative considerations to offer. I shall not be so presumptuous as even to think of lifting the veil that envelops the future; on the contrary, I shall adduce considerations which will serve rather as admonitions against positive and hasty conjectures concerning the future development of science.

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If we scrutinise the development of theory closely, it will be immediately apparent that its course has been by no means so continuous as we might be inclined to believe, but rather that it is full of gaps and has not taken place, to appearances at least, along the simplest and most logical paths. Certain methods have frequently yielded the most beautiful results, and many persons have been tempted to believe that the development of science to the end of all time would consist in the systematic and unremitting application of them. But suddenly they begin to show indications of impotency, and all efforts are then bent upon discovering new and antagonistic methods. Then there usually arises a conflict between the adherents of the old method and those of the new. The point of view of the former is characterised by its opponent as antiquated and obsolete; whilst its upholders in their turn look down with scorn upon the innovators as perverters of true classical science.

This is a process, moreover, which is by no means restricted to theoretical physics, but to all appearances recurs in the history of every field of intellectual activity. Many doubtless believed in the days of Lessing, Schiller, and Goethe that the dramatic literature of the future was contained in all its possible manifestations potentially in the ideal poetical methods cultivated by these great masters; whereas to-day totally different methods of dramatic writing are employed, and the correct method has possibly not yet been reached.

In like manner in art the Impressionists and Secessionists stand arrayed against the old schools of painting, and the Wagnerian school of music against the schools of the ancient classical mas-

ters. There is accordingly no occasion for surprise that theoretical physics does not form an exception to this general law.

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Building upon the labors of a number of gifted predecessors, Galileo and Newton erected a system which may be characterised as the real beginning of theoretical physics. Newton added to this system his great theory of the motion of the celestial bodies. He considered each of these bodies as a mathematical point, as, indeed, each of the fixed stars appears to be to the unaided eye. Between each two of these points a force of attraction acting inversely as the square of the distance and in the direction of the line of junction was conceived to operate. Assuming a like force as operative between every two of the material particles of bodies generally and applying the laws of motion which he had derived from the observation of terrestrial objects, Newton succeeded in deducing from one and the same law the motions of all the celestial bodies, the phenomena of gravitation, the tides, etc.

In consideration of Newton's marvellous success, the endeavours of his followers were bent toward explaining all the other phenomena of nature exclusively by Newton's method, under appropriate modifications and extensions. Starting from an ancient hypothesis which dates back to Democritus, they conceived bodies as aggregates of great numbers of material points called atoms. Between each two of these material points, there was supposed to act, besides the Newtonian attraction, a supplementary force, which at certain distances repelled and at others attracted, according as the requirements of explanation demanded.

The so-called principle of living forces was a contemporary contribution of mathematical inquiry. Every time that work is performed, that is to say, every time the point of application of a force is displaced through a certain distance in the direction of the force, a definite quantity of motion is supposedly produced, a quantity which is measured by a mathematical expression to which the name of *living force* was given. Exactly this quantity of motion does in reality make its appearance when the force acts uniformly upon all the particles of a body, as in the case of free descent,

but it makes its appearance in less amount when the forces act upon a few particles only and not upon the remainder, as in the case of friction and impact. In all occurrences of the last-named kind heat is generated instead. The hypothesis was then formed that heat, which prior to this had been regarded as a substance, was nothing less than an irregular relative motion of the smallest particles of bodies with respect to one another,—a motion which cannot be directly seen, since the particles in question are themselves invisible, but which is communicated to the minute material constituents of our nerves and evokes thus the sensation of heat.

The truth of the theory that the heat generated is in every case of necessity exactly proportional to the living force which is destroyed, a theory which was termed the principle of the equivalence of living force and heat, was in the outcome confirmed. Then the additional supposition was made that in solid bodies each and every particle oscillates about a definite position of equilibrium and that the configuration of these positions of equilibrium was determinative of the solid conformation of the body. In the case of liquids, these molecular movements were so brisk that the particles slipped by one another; evaporation was produced by the complete separation of the particles from the superficies of bodies, so that in gases and vapors the particles sped along on their paths mostly in rectilinear directions like bullets shot from a gun. Thus, for example, was explained with facility the occurrence of bodies in their ordinary three aggregate states, as were likewise many facts of physics and chemistry. It was a direct consequence of many of the properties of gases that their molecules could not possibly be material points; it was therefore supposed that they were more complicated aggregates of such points, possibly enveloped by integuments of ether.

In addition to the ponderable atoms constituting bodies, there was assumed the existence of a second substance composed of atoms much more minute,—namely, the luminiferous ether, by the transversal undulations of which were explained nearly all the phenomena of light which Newton had formerly ascribed to the emanation of specific luminous particles. There were still a few

difficult problems remaining, like that of the utter absence of longitudinal waves in the luminous ether,—a form of undulatory motion which not only occurs in all ponderable bodies, but actually plays the leading part in their existence.

Our knowledge of the facts of electricity and magnetism were enormously extended by Galvani, Volta, Oersted, Ampère, and others, and had been advanced by Faraday to virtual completion. With comparatively meager resources the latter inquirer had unearthed so marvellous a plenitude of facts that it long seemed as if the task of the future would be entirely restricted to the elucidation and application of his discoveries.

Specific electric and magnetic fluids had long been conceived as the effective causes of the phenomena of electromagnetism. Ampère succeeded in explaining the phenomena of magnetism by means of molecular electric currents, thus rendering the assumption of magnetic fluids superfluous, while Wilhelm Weber so completed the theory of electric fluids as to simplify greatly the explanation of all electromagnetic phenomena hitherto known. To this end, he conceived the electric fluids to be composed of minute particles, precisely as were ponderable bodies and the luminiferous ether; and between these electric particles he conceived forces to act precisely similar to those which were operative between the particles of other substances, with the sole modification, in itself unessential, that the forces acting between every two electric particles were also determined by the relative velocities and accelerations.

Whereas thus in the preceding periods inquirers had assumed, in addition to sensible matter, a caloriferous substance, a luminiferous substance, two magnetic and two electric fluids, etc., now ponderable matter, the luminiferous ether, and the electric fluids were found to suffice. Each of these substances was conceived to be composed of atoms, and the peculiar task of physics for all future time appeared to be definitively restricted to determining the law of operation of the *actio in distans* obtaining between each pair of atoms and subsequently to integrating under the appurtenant initial

conditions the equations which flowed from these various interactions.

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This was the state of development of theoretical physics when I began my studies. What marvellous changes have since been wrought! When I look back over the manifold developments and transformations that have taken place, I seem to myself like a veteran on the field of science; nay, I might even say that I alone am left of those who embraced the old doctrines heart and soul; at least I am the only one who is still sturdily battling for them. I look upon it, in fact, as the mission of my life to do my utmost, by clear, logical, and systematic exposition, to render the permanent and useful acquisitions of the old classical theory so secure that they shall not have to be discovered a second time,—a phenomenon which is not of isolated occurrence in the history of science.

I appear before you, therefore, as a reactionary and belated thinker, as a zealous champion of the old classical doctrines as opposed to the new. Nevertheless, I am convinced that I am no narrow-minded partisan, blind to the excellencies of the new theories, to which justice shall be rendered in the following pages as far as lies in my power; for I am only too well aware that like others I also see the things of this world as colored by the glasses of my own subjectivity.

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The first onslaught upon the scientific system above described was directed toward its weakest point; namely, Weber's theory of electrodynamics. This was in a sense the flower of the intellectual labors of this gifted inquirer, who won undying renown by his numerous theoretical and experimental researches in the field of electrodynamic measurement. Yet, despite its great ingenuity and mathematical refinement, Weber's theory bears so distinctly the stamp of artificiality that but a few enthusiastic adherents doubtless ever reposed implicit confidence in it. Maxwell, an unqualified admirer of the labors of Weber, led the assault.

The labors of Maxwell enter into consideration here under two

points of view: first, under their epistemological aspect and secondly under their purely physical aspect. As to the first point of view, Maxwell distinctly indicates the danger involved in holding a theory of nature to be absolutely and exclusively correct because many consequences that follow deductively from it may happen to be confirmed by experience. He shows by numerous instances that one and the same group of phenomena frequently admits of explanation in two totally different ways; each method of explanation representing the facts as well as the other. It is not until new and hitherto unknown phenomena are adduced that the advantages of the one theory over the other are apparent, and even then the victorious theory may, on the discovery of further fresh facts, have to yield to a third theory.

Whilst the creators of the old classical physics, and far more so their successors, imagined that they had revealed the intimate nature of phenomena by their explanations, Maxwell claimed no more for his theory than its being a mere constructive representation of nature, or "mechanical analogy" as he terms it, enabling the imagination to depict in the simplest manner possible at the time all the phenomena concerned. We shall see what a beneficial effect this attitude of Maxwell's had on the further development of his theory. The victory for his theoretical ideas was immediately secured by their practical consequences.

We saw that all known electromagnetic phenomena had actually been explained by Weber's theory, which supposed electricity to consist of particles which acted directly upon one another at any distance without any intermediate agency. Stimulated by the ideas of Faraday, Maxwell constructed a theory which proceeded from a directly opposite point of view. According to his theory, every electric or magnetic body acted only upon the immediately adjacent particles of a medium filling all space, which particles acted in turn upon the next adjacent particles of the medium, until the action had been communicated to the neighboring body.

Known phenomena were explained as well by the one theory as by the other; but Maxwell's theory extended farther than the old one. According to his view, as soon as electric displacements

could be generated in sufficiently rapid succession, there would necessarily be produced in the medium, a wave motion which would exactly obey the laws governing the undulations of light. Maxwell accordingly surmised that rapid electric disturbances always took place in the particles of luminous bodies and that the vibrations thus evoked in the medium were what we know as light. The medium which is the vehicle of electromagnetic action is thus recognised as identical with the old luminiferous ether, and we may consequently give to it the same name, although it must necessarily possess many other properties in order to serve as the vehicle of electromagnetism.

The reason that no vibrations of this character were observed in the early experiments with electricity may perhaps be rendered plain by the following illustrations: Place the palm of your hand against a pendulum at rest; raise the pendulum slowly by gently pressing against it, and then let the hand fall slowly back in the opposite direction, finally withdrawing it altogether. The pendulum will follow the hand and execute a half vibration, but it will not swing up on the other side because the velocity imparted to it is too slight. The following may also serve as an illustration: Theory assumes that on plucking a stretched string, a point of the string is pulled out of the position of equilibrium, and then suddenly the entire string is left to itself. I did not believe this as a young student, but thought that the person plucking the string ought to impart to it an additional impulse, for in my first experiment in pushing the string aside with my finger and then withdrawing it in the direction in which the string would have vibrated, the latter emitted no sound. I did not see, that compared with the great velocity of oscillation of the string the movement of my finger was exceedingly slow, and so checked the movement of the string.

In like manner, in the old experiments the electrical oscillations were in all cases performed too slowly as compared with the enormous velocity of propagation of electricity. After many difficult preliminary experiments, the leading ideas of which he has himself set forth in the frankest manner, Hertz found certain experimental conditions in which electric states could be periodically

altered so rapidly that measurable waves were produced. Like all the productions of genius, their simplicity is remarkable, yet for reasons which are quite apparent I cannot enter here upon their detailed consideration. The waves which Hertz produced by electric discharges differ, as Maxwell had predicted, in no qualitative respect whatever from waves of light; but quantitatively the difference is prodigious.

As pitch in sound, so color in light is determined by the rapidity of the vibrations. In visible light about four hundred million million vibrations a second in the outermost red, and eight hundred million million in the outermost violet, are the extreme limits of the rates of vibration. For a long time it had been known that there were ether-waves of like nature having rates of vibration some twenty times less than those in the outermost red and some three times as great as those in the outermost violet. These are invisible to the eye, but the first or so-called ultra-red are perceptible by their thermal effects and the latter or the so-called ultra-violet by their chemical and phosphorescent effects. In the waves produced by Hertz by actual discharges, there took place in a second not more than about one thousand million vibrations, and Hertz's successors produced waves having a hundred times as many vibrations per second.

It stands to reason that vibrations that take place so slowly as compared with luminous vibrations cannot be seen directly by the eye. Hertz demonstrated their existence by means of microscopic sparks generated by them in appropriately-shaped conductors at great distances. These conductors may therefore be correctly termed "eyes" for Hertz's vibrations. With these means, Hertz confirmed Maxwell's theory in its minutest details, and although it was attempted to explain electric vibrations by the theory of action at a distance, the superiority of Maxwell's theory was soon universally admitted; indeed, the pendulum swung so far to the other side that the extremists ultimately came to speak of the incompetency of all the conceptions of the old classical theory of physics. But of this later. We shall first stop to speak a little more at length of these brilliant discoveries.

Of the various ether-waves that had been discovered before Hertz's time it was known that some passed more easily through one class of substances and others more easily through another. Thus, an aqueous solution of alum permits the passage of all visible rays but of only a few ultra-red rays; these ultra-red rays, on the other hand, penetrate solutions of iodine in carbon disulphide, which are absolutely impervious to visible light. Hertz's waves pass through almost all bodies with the exception of metals and electrolytes. Accordingly, when Marconi produced very short Hertzian waves in one place and translated them into the Morse alphabet at another several miles distant by means of an instrument which was but a modification of the apparatus which we call the "eye" for Hertz's waves, what he did was nothing more than to construct an ordinary optical telegraph; with this difference, that instead of employing waves of five hundred million million vibrations per second, he employed waves having but about one hundred thousand million vibrations in a second. The advantage of the last-named waves is that they pass with very slight diminution of power through fogs and even through masses of rocks. But they would no more be able to pass through a mountain of solid metal or through a fog of mercury globules than visible light would through an ordinary mountain or fog.

The variety of the forms of radiant energy known to us was still further enriched by the justly celebrated discovery of Roentgen's rays. These pass through all bodies, including metals, although in the latter case, as well as in that of metaliferous bodies and calcareous bones, their power is considerably diminished. The phenomena which had been demonstrated in connexion with all former kinds of radiant energy, namely, polarisation, interference, and refraction, were not observed in connexion with Roentgen's rays. If these were actually incapable of polarisation they would, if they were waves at all, necessarily be longitudinal. But the possibility even exists that they are incapable also of interference, and hence are not waves at all, which is the reason for our caution in speaking of Roentgen *rays* and not of Roentgen *waves*. If ever a body were discovered capable of polarising them, we should have

reason to regard them as qualitatively identical with light, but even in that event they would have to have a very much shorter period of vibration than even the outermost ultra-violet rays, or might possibly be made up, as some physicists are inclined to believe, of impulses following one another in rapid succession.

In view of this prodigious variety of form which rays exhibit, we might be inclined to pick a quarrel with our creator for having made our eye sensitive to so small a portion only of this vast domain; but as in all such cases we should do so unjustly, for never is more than a tiny portion of the great All of nature revealed directly to man, though in compensation his intellect has been rendered competent to acquire the knowledge of the rest by suitable effort.

If the Roentgen waves really were longitudinal waves of the luminiferous ether, which their discoverer was at first quite disposed to believe, and which has so far not been disproved by a single fact, we should be confronted with an interesting but by no means isolated incident in the history of science. The classical theoretical physics long ago perfected its views regarding the composition of the luminiferous ether. One thing only was wanting, as it was believed, for irrevocably confirming its correctness, namely, the presence of longitudinal waves in the ether. But these could not be made to appear. Now, after it has been proved that the luminiferous ether must have a substantially different constitution, since it is also the vehicle of electric and magnetic action, now I say, after the old theory of the constitution of the luminiferous ether has been exploded, we are on our hypothesis brought, *post festum* as it were, to the very verge of the long-desired confirmation, the discovery of the existence of longitudinal waves in the ether.

The case of Weber's theory of electrodynamics was similar. This theory rested, as we saw, upon the assumption that the action of electric charges depends on their relative motion, and just at the moment when the insufficiency of Weber's theory was definitively demonstrated Rowland found in Helmholtz's laboratory by direct experiment that moving charges of electricity act differently from

charges at rest. Formerly, scientists would have been inclined to accept this experimental result as a direct proof of the correctness of Weber's theory, but to-day it is known that it is not an *experimentum crucis*, but that it follows also from Maxwell's theory.

Furthermore, it follows from a modification of Weber's theory that not only the conductors carrying the currents but also the currents within the conductors must be deflected by the magnet. This phenomenon also, after having been long sought in vain, was discovered by the American physicist Hall at a time when the adherents of Weber's theory had suffered so many decisive defeats that even the thought of triumph over the new discovery was impossible.

Such things demonstrate the great caution that is necessary when one is tempted to look upon the confirmation of a consequence of a theory as a proof of its unconditional correctness. According to Maxwell's view, our mental representations, which have been made to conform to nature in certain instances, prove often to be automatically in accord with nature at many other points; but it does not necessarily follow from this that they are in accord with her at all points. The same considerations also go to show that even a wrong theory may prove of value by stimulating inquirers to new experiments.

It was demonstrated by the above-mentioned discoveries of Hertz, Roentgen, Rowland, and Hall, that Faraday had after all left something for his successors to discover. To these may be added a number of other recent discoveries, of which we shall mention here only the phenomenon of Zeeman with respect to the action of magnetism on the emission of light and the corresponding phenomenon of the action of magnetism on the absorption of light. All these phenomena, of which many were actually sought after by Faraday, could not have possibly been reached by the means at that investigator's disposal. Genius has frequently accomplished wonderful results with insignificant resources, but it is no less true that the human mind could never have achieved some of its noblest conquests save for the marvellous perfection to which physical apparatus and physical experimentation have been advanced in recent times.

The majority of the novel phenomena here described have been investigated as yet only superficially. The study of their details and of their relations to one another and to other known phenomena, their elaboration in the mechanical loom of physics, if I might hazard the phrase, opens for future generations a field of research which is apparently immeasurable. The many practical results which have already been obtained from them, at the very outset, as it were, (for example, the X-ray photography, wireless telegraphy, and radiotherapy,) give some inkling of the vast wealth of practical consequences which will be forthcoming upon a thorough-going exploitation of the details. But theory has been hard put to it by the new facts. The intellectual tranquillity into which she lapsed from her belief that she had comprehended everything has been rudely shaken, and no attempt has yet been successful to bring the new phenomena under so simple and satisfactory a point of view as the old. In fact, everything is still in a state of vacillation and fermentation.

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This state of confusion was augmented by the combined influence of several other factors. In the first place are to be mentioned certain philosophical criticisms of the foundations of mechanics, which have been most distinctly formulated by Kirchhoff. The dualistic notions of force and matter had been unhesitatingly adopted by the old mechanics. Force was considered as an independent agent existing by the side of matter and the cause of all motion. Nay, the dispute even arose as to whether force existed at all in the sense that matter did, whether it was not rather a property of matter, or contrariwise whether matter should not be regarded as a product of force.

Kirchhoff was far from desiring to answer these questions. Doubtless he looked upon the entire method of formulating the problem as futile and inept. Yet in order to be dispensed from expressing his opinion upon the value of such metaphysical inquiries he declared it his purpose to eschew absolutely these obscure notions and to confine the task of mechanics to the simplest and most

unequivocal possible description of the motions of bodies, without consideration of their metaphysical origin. In his *Mechanics*, therefore, he speaks only of material points and of the mathematical expressions by which the laws of their motion are formulated; the notion of force is omitted altogether. Napoleon once exclaimed in the vault of the Capuchins at Vienna: "All is vanity save force;" but Kirchhoff in a single page of printed matter absolutely eliminated force from nature, putting to shame even that German professor of whom Karl Moor relates that despite his physical impotency he had the audacity to treat of the nature of force yet not to destroy it.

Kirchhoff afterwards reintroduced the word force, but only as an abbreviated designation for certain algebraic expressions which constantly occur in the description of motion, and not as a metaphysical notion. The attempt was afterwards repeatedly made to enhance the significance of this word, especially with reference to the analogy afforded by the feeling of muscular effort which is so familiar to man, but the old obscure formulations and conceptions will doubtless never recur again in science.

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It was not Kirchhoff's purpose to alter in any way the subject-matter of the old classical mechanics; his reformation was a purely formal one. Hertz went much farther; and while nearly all subsequent authors have closely followed Kirchhoff's mode of exposition, sometimes imitating, unfortunately, his phraseology rather than his spirit, no such destiny has been allotted to Hertz's ideas. I have frequently heard Hertz's mechanics highly praised, but so far I have seen no one treading in the paths which he marked out.

To my knowledge, attention has never been called to the fact that a characteristic conception of Kirchhoff's mechanics, pushed to its last consequences, leads directly to the ideas of Hertz. Kirchhoff limits his definition of the most important concept of mechanics, that of mass, altogether to the case where arbitrary equations of condition exist between the material points. In this case the necessity of the factor designated by him as mass is clearly

seen. In the remaining cases, where the motion of the material points is not restricted by equations of condition, as in the old effects produced by forces, (for example, in the theory of elasticity, in aeromechanics, etc.,) Kirchhoff's concept of mass is intangible and the consequent obscurity is entirely removed only by excluding the last-mentioned cases.

This Hertz did. The most important forces of the old mechanics were forces acting directly at a distance between every two material particles. The question of the metaphysical cause of this action at a distance Kirchhoff abolished from mechanics; but he admitted motions which took place exactly as if these forces were in existence. Now, as we have seen, the conviction prevails to-day that electric and mechanical effects are transmitted by the agency of a medium. There remains thus naught but molecular forces and gravitation, which last even its discoverer Newton assumed to be probably due to the action of a medium. The molecular forces may approximately be replaced in solid bodies by the condition of invariability of form, and in liquid bodies by the invariability of volume. And while no successful attempt has as yet been made to replace elasticity, the expansive force of compressible liquids, chemical forces and forces of crystallisation, by like conditions; yet Hertz was doubtless convinced that such efforts would ultimately be crowned with success, and advancing beyond Kirchhoff he rejected even motions which took place as if the old forces existed, admitting only motions for which such conditions obtained as complied with certain formal mathematical definitions which he laid down. The only other principle which he employs for the construction of his mechanics is a law of motion which represents a special case of Gauss's principle of least constraint.

In sum, Kirchhoff merely interdicted inquiry as to the *causes* of the motions that had formerly been ascribed to forces acting at a distance, while Hertz abolished these motions themselves and sought to explain forces by equations of condition, thus directly inverting the method that had formerly prevailed of explaining the conditions of motion by forces. Hertz, therefore, made a far more serious attempt than Kirchhoff to conquer the difficulties inhering

in the concept of force. And he created in so doing a remarkably simple system of mechanics, which flows from very few principles, alike logical and natural in their character. Unfortunately, at the very moment of its enunciation, his lips were sealed forever in death, and the thousands of questions which certainly not I alone would fain have directed to him for enlightenment, must remain unanswered.

It will be understood from what has been said that certain phenomena, such as the free movement of rigid systems, follow readily from Hertz's theory. For other phenomena Hertz is obliged to assume the existence of invisible masses. The motions of these invisible masses determine by their interposition the motions of the visible masses, rendering the explanation of their laws of motion possible; they are consequently the counterparts of the invisible medium that produces electromechanic and gravitational phenomena. But what shape are these unknown elemental masses to take in our minds? And is it always possible for us to reach our goal by their agency? It is not permissible to assign to them the structural composition of the old media or even that of the luminiferous ether of Maxwell, for the reason that in all these media the precise forces are supposed to act which Hertz excluded.

Even in the very simplest mechanical problems the systems of invisible masses required by Hertz's theory are disproportionately intricate; and I am consequently inclined to attribute to this theory a purely scholastic value only.

Hertz's mechanics appears to me, therefore, more in the nature of a program for the distant future. If we should ever be successful in explaining in a simple manner all the phenomena of nature by such invisible motions as Hertz has described, then the old mechanics would be displaced by Hertz's system. But until that time comes the old system is the only one which is qualified to explain all the phenomena lucidly and satisfactorily without recourse to agents which are not only concealed from our sight but are also absolutely beyond our range of conception.

In his book on mechanics Hertz gave in a sense the finishing touch not only to the mathematico-physical ideas of Kirchhoff, but

also to the epistemology of Maxwell. Maxwell termed the hypothesis of Weber a *real* physical theory, meaning that its author claimed for it objective validity, whereas he ascribed to his own performance merely the value of constructive representations of phenomena. Starting from this point, Hertz impresses very distinctly upon the minds of physicists a principle with which philosophers had doubtless long been familiar, that no theory represents something absolutely objective, something absolutely coincident with nature, but that on the contrary every theory is a mental representation only of phenomena, and that it bears the same relation to the things it represents as a symbol does to the things it symbolizes.

It follows from this that the object set us is not to discover an absolutely correct theory, but rather to light upon some constructive model which shall be as simple as the circumstances admit and represent the phenomena most adequately. In fact, it is not inconceivable that two quite different theories should exist which are equally simple and which accord equally well with the phenomena, and which therefore, although they are totally different, are yet equally correct. The assertion that a given theory is the only correct one is merely the expression of our subjective conviction that there is no other theory so simple and according so well with the facts.

Numerous questions which formerly appeared unfathomable are rendered nugatory from this point of view. How is it possible, it was said formerly, for a force to emanate from a material point, which is nothing but an intellectual entity? How can any combination of points furnish extension? And so on. Now we know that not only forces, but material points also, are mere mental representations. The points cannot possibly be equivalent to extended objects, but they may represent them to any degree of approximation we wish. The question whether matter is composed of atoms or is a continuum, is reduced to the far clearer question of whether the concept of an enormously large number of individual entities is calculated to furnish a more perfect mental representation of phenomena than the concept of a continuum.

We have been speaking in the main of mechanics. A revolution which affected the entire domain of physics, as distinguished from mechanics proper, was inaugurated in connexion with the rapid growth in import and scope of the principle of energy. We have already referred incidentally to this principle as one of those deductive consequences of the mechanical philosophy which had been verified by experience. According to this philosophy, energy is a perfectly familiar mathematical expression, absolutely devoid of mysticism and composed in quite definite manner of magnitudes which had been admitted earlier into the science (namely, mass, velocity, force, and distance); and since the philosophy in question regards heat, electricity, etc., as forms of motion, even while granting that these forms are as yet imperfectly known in character, it was inclined to see in the principle of energy merely an important confirmation of its conclusions.

Evidence of appreciation of this principle is met with, in fact, in the very infancy of mechanics. Leibnitz spoke of the substantial nature of force, by which he meant energy, in language which might have been used by the most recent champions of the doctrine. In inelastic impact, however, deformation, disruption of coherence and texture, the bending of springs, etc., are conceived as the products of living force; that heat is a form of energy Leibnitz has not the slightest inkling. As to the facts, therefore, Du-bois Reymond is entirely in the wrong when he again seeks to belittle Robert Mayer in his commemorative address on Helmholtz, and denies to Mayer the priority of the discovery of the equivalence of heat and mechanical work. As for Mayer, he was by no means an advocate of the view that heat was a motion of molecules; he looked upon heat rather as an entirely new form of energy and contended merely for its equivalence with mechanical energy. Even the physicists who espoused the molecular theory, foremost among them Clausius, sharply distinguished between the theorems which follow exclusively from this view (special thermodynamics) and those which are deducible from established facts of experience independently of any hypothesis regarding the nature of heat (general thermodynamics).

But special thermodynamics, after a succession of brilliant discoveries, was suddenly checked in its career by the difficulties encompassing the mathematical treatment of molecular motions. General thermodynamics, on the other hand, achieved remarkable results. It was found that the temperature determined the occasion and the proportion in which heat and work are transformed. The increment of heat added turned out to be the product of the so-called absolute temperature into the increment of a second function which after Clausius was termed entropy. From this were deduced, principally by Gibbs, new functions, like that which afterwards received the name of thermodynamic potential at constant temperature, constant pressure, etc., and by their assistance the most surprising results were reached in the most varied fields, as in chemistry, capillarity, etc.

It was found, further, that equations of analogous form obtained also for the transformation of other forms of energy, electric, magnetic, radiant, etc., into one another; and that in particular it was also possible to decompose each form of energy into two factors,—a procedure which was attended with no less success. This roused the enthusiasm of a certain group of inquirers, who styled themselves energeticians, to such a pitch that they immediately declared for absolute rupture with the old ideas, contending that it was fallacious to infer the identity of heat and living force from their equivalence, apparently unaware that the theorem of equivalence was not the only argument in favor of this proposition.

The concept of energy is regarded by the new physical philosophy as the only correct point of departure for physical investigation. The principle that each form is decomposable into two factors, together with the theorem of variation connected with it, are regarded as the fundamental law of all nature. Every mechanical illustration designed to explain why energy assumes such and such curious forms and follows in each of them substantially different, though analogous, laws, they regard not only as redundant but even as detrimental; and the problem of physics, nay, of all natural science, for all time to come, is conceived by them to consist exclusively in the description of the deportment of energy in all its

forms, to be in fine a natural history of energy,—an ideal which, if the totality of natural action go by the name of energy, is nothing less than tautological.

The analogies in the deportment of the different forms of energy are beyond question of the greatest significance and interest, and their systematic study must be considered as one of the most beautiful tasks of physics. The importance of the concept of energy also doubtless justifies the attempt to select this principle as a starting-point. And it must further be admitted that the form of research designated classical theoretical physics frequently led to extravagances against which some sort of reaction was imperative. Every Tom, Dick, and Harry felt himself called upon to devise his own special combinations of atoms and vortices, and fancied in having done so that he had pried out the ultimate secrets of the Creator.

I know how helpful it is to consider the problems of science under the most varied aspects, and I have but the warmest sympathy for every original and enthusiastic undertaking in science. I therefore cordially extend to the secessionists my hand. But I was convinced that the energeticians had often suffered themselves to be deceived by superficial and merely formal analogies; that their laws lacked that lucidity and distinctness of form, and their deductions that rigor, which characterised the classical physics; and that they had discarded many elements of the old doctrines that were helpful, nay, even indispensable, to science. And furthermore, the controversy as to whether matter or energy was the only existing reality appeared to me to be a decided relapse to the old metaphysical point of view which we believed we had overcome, and a violation of the principle that all theoretical concepts are constructive images only.

In expressing without reserve my conviction on all these points, I imagined that I was demonstrating, in a far more helpful manner than by praise, my interest in the future development of the doctrine of energy. As in Hertz's mechanics, so in the doctrine that the entire body of physics is deducible from the two component factors of energy and the allied law of association, I

can accordingly discern nothing more than an ideal of the distant future. The latter alone can determine the question, which is to-day absolutely undecided, whether such a constructive theory of nature is better than the old, or is the best obtainable.

* * *

From the energeticians we come to the phenomenologists, who might be termed the moderate secessionists. Their philosophy arose as a reaction against the predominating tendency of the old view to regard the hypotheses concerning the composition of atoms as the real aim of scientific inquiry and the laws for visible phenomena resulting from these hypotheses as merely a means for controlling the same.

True, this tendency characterised only the extremest branch of the old school. We saw that even Clausius had made a rigorous distinction between general thermodynamics, which was independent of molecular hypotheses, and special thermodynamics. Many other physicists also, as for example Ampère, Franz Neumann, and Kirchhoff, dispensed with molecular theories in their deductions, although they did not deny the atomistic structure of matter.

There is very frequently found among the adherents of the old school a method of deduction which I would fain call the Euclidean, seeing that it is modelled upon the method of Euclid's geometry. A few theorems (axioms) are assumed either as self-evident or as established by experience; from these, certain simple elementary laws are then deduced as logical consequences, and from these finally the universal or integral laws are constructed.

This method, in combination with the ordinary deductive procedures of the molecular theory, seemed to satisfy tolerably well all the requirements that had hitherto been demanded of inquiry; but not so with Maxwell's theory of electromagnetism. In his first works, Maxwell conceived the medium that transmitted electromagnetism to be made up of a large number of molecules or at least individual mechanical units; but their structure took so complicated a form that they could not possibly lay claim to any

other validity than that of auxiliary conceptions for the discovery of equations, or that of ideal mechanisms producing effects in some way analogous to the phenomena of reality, certainly not that of definitive facsimile representations of what actually took place in nature. Subsequently, Maxwell showed that many other mechanisms besides those mentioned were capable of leading to the desired goal, provided they satisfied certain general conditions; but all endeavors to find some definite and very simple mechanism embodying all these conditions were fruitless. This paved the way for a doctrine which I believe may be most trenchantly characterised by our reverting a third time to Hertz's ideas. These, as found in the introduction to his treatise on the fundamental equations of electrodynamics, are quite typical of this theory.

Hertz did not seek for, at any rate did not find, a satisfactory mechanical explanation of these fundamental equations; but he also disdained to use the Euclidean mode of deduction. He correctly calls attention to the fact that in mechanics it is not the few experiments from which the mechanical equations are ordinarily derived, nor in electrodynamics the five or six fundamental experiments of Ampère, that have established in our minds the unshakable conviction of the truth of all these equations, but that rather it has been the subsequent complete agreement of these equations with the hitherto known facts of reality. He then delivers the Solomonic dictum that the wisest course is, after the equations are once in our possession, to write them down without any solicitude as to their deduction, and afterwards to compare them with the phenomena and to discern in their unvarying concordance with the facts the best demonstration of their truth.

The view here expressed in its extremest form met with the most varied reception. Some seemed inclined to look upon it as a piece of pleasantry. Others took the matter more seriously. Eschewing utterly the assistance of hypotheses and of visual or mechanical illustrations, they made it the sole goal of physics to write down for every group of phenomena the equations by means of which their behavior could be quantitatively calculated; its sole business consisted for them in the discovery by trial of the

simplest possible equations that fulfilled certain necessary formal conditions, as of isotropy, etc., and in the subsequent comparison of these equations with experience. This opinion is represented by the extreme wing of the phenomenologists, which I am tempted to call the mathematical wing. Mathematical phenomenology differs from general phenomenology. The latter seeks to describe every province of facts after the manner of natural history, by enumerating and delineating all the phenomena belonging to the province, without limitation of the means to be employed, but excluding any set philosophical doctrine, mechanical explanation, or other foundation. It is characterised by Mach in the statement that electricity is nothing more nor less than the sum-total of the experiences which have already been made in this domain and which there is hope of making in the future. Both branches of this school make it their aim to represent phenomena without going beyond experience.

Mathematical phenomenology fulfils primarily a practical need. The hypotheses by which the equations were originally reached were found to be precarious and subject to change; but the equations themselves, after they had been put to experimental test in a sufficiently large number of cases, remained intact, at least within certain limits of exactitude, beyond which they were in need neither of perfection nor refinement. If for practical purposes only, then, it is eminently desirable to divorce as absolutely as possible what is stable and established from what is vacillating and unfirm.

It must further be admitted, that the object of every science, and therefore also of physics, is perfectly attained when formulæ are found by which we can calculate in advance with uniqueness, certainty, and absolute accuracy, the phenomena which are going to happen. But it is to be remembered that this is just as unattainable an ideal as a knowledge of the laws of action and of the initial states of all atoms.

The assumption of phenomenology that nature can be represented mentally without proceeding at any point beyond experience, is in my opinion an illusion. No equation represents phenomena with absolute exactitude; they all idealise the phenomena;

they all emphasise the common features of the phenomena and neglect the divergent; they all, therefore, transcend experience. That this is necessary if our object is to attain conceptions that shall enable us to predict the future, follows from the very nature of thinking itself, which is but a process of adding something to experience and of creating a mental construct which is not experience and which therefore can represent many experiences.

Experience, says Goethe, is never more than half of experience. The more boldly we advance beyond experience, the broader the survey we obtain, the more surprising the facts we discover, but the greater the likelihood of our going astray. Phenomenology, therefore, should not make it its boast that it does not transcend experience, but should merely admonish inquirers not to transcend it too far.

The phenomenologists are also in error when they fancy that they have substituted no constructive images for nature. Numbers, their various relations and combinations, are as much constructive images as the geometric representations of mechanics. They are less exuberant, they are better adapted to quantitative representation, but on the other hand they are illly qualified for opening up new perspectives; in fine, they are very poor guideposts for discovery. And so, all the other conceptions employed by general phenomenology prove to be constructive images of phenomena. The best results, therefore, will doubtless be obtained by the employment of all the various methods of representation, each according as it is needed; care being taken to put our representations to fresh experimental tests at every step.

If this be done, there will be no danger of our overlooking facts through prepossession with our theories, as the atomists have been reproached with doing. This is a danger to which every theory is exposed, no matter what its character, when its special bias is pushed to the extreme. The difficulty therefore was due less to any distinctive feature of atomism than to the fact that inquirers had not been sufficiently forewarned against placing implicit confidence in their theories. It is no less impermissible for the mathematician to confound his formulæ with the truth, unless he also

would run the risk of becoming blinded to the facts. This is seen in the case of the phenomenologists when they refuse to take note of so many facts that are intelligible from the point of view of special thermodynamics alone; in the case of the opponents of atomism when they absolutely ignore everything that speaks in favor of their doctrine; nay, even in the case of Kirchhoff when, relying upon the applicability of his hydrodynamic equations, he held the inequality of pressure at different points of a heat-conducting gas to be impossible.

The mathematical phenomenologists naturally reverted to the concept of the continuity of matter, which had the support of appearances. I called to their attention the fact that by definition the differential equations which they used represented transitional limiting states, which were totally devoid of meaning without the assumption of a very large number of individual entities. An unthinking use of mathematical symbols only could ever have led us to separate differential equations from atomistic conceptions. As soon as it is clearly seen that the phenomenologists, under the veil of their differential equations, also proceed from atomistic entities, which they are obliged to conceive differently for every group of phenomena and as endowed now with these and now with those complicated properties, the need of a simplified and uniform atomistic doctrine will soon be felt again.

The energeticians and phenomenologists attributed the decline of the molecular theory to its unfruitfulness. Whereas this theory, in the opinion of some, had never done aught but injury, others admitted that it had originally been of great utility and that nearly all the equations which are now regarded by the mathematical phenomenologists as constituting physics were obtained by the methods of the molecular theory. The only contention of the phenomenologists was that now that we had acquired these equations the molecular theory was superfluous. All vowed its annihilation. They appealed to the historical principle that frequently opinions which are held in the highest esteem have been supplanted within a very short space of time by totally different theories; nay, even as St. Remigius the heathens, so now they exhorted the theoretical phys-

icists to consign to the flames the idols that but a moment previously they had worshipped.

But historical principles are sometimes double-edged weapons. History does no doubt often show revolutions which have been unforeseen; unquestionably, it is profitable to bear in mind the fact that what now seems to be most firmly established may possibly be supplanted by something entirely different; but it should also be borne in mind that some achievements may possibly remain the possessions of science for all time, though in a modified and perfected form. Indeed, by the very historical principle in question a definitive victory for the energeticians and phenomenologists would seem to be impossible, since their defeat would be immediately required by the fact of their success.

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Following the example of Clausius, the adherents of special thermodynamics never disputed the high import of general thermodynamics; the successes of the latter science, therefore, prove nothing against the validity of the first. The question simply is whether there are not additional results which atomism only could have reached, and of such results the atomistic theory has had many remarkable specimens to show, even long after the period of its greatest glory. From the principles of purely molecular physics Van der Waals deduced a formula which gives the behavior of liquids, gases, and vapors, as well as of the various transitional forms of these aggregate states, with admirable approximation if not absolute decision, and which has led to many new results, as, for example, the theory of corresponding states. Considerations derived from molecular physics have just recently indicated the way for the improvement of this formula, and the hope is not a forlorn one that the behavior of the simplest chemical substances, namely argon, helium, etc., may soon be represented with absolute exactness; so that it is precisely the atomistic theory that has approached nearest to the ideal of the phenomenologists, namely that of a mathematical formula which shall embrace all physical states. To this has been added a kinetic theory of liquids.

Just as it formerly shed light on Avogadro's law, on the nature of ozone, etc., so recently the atomistic theory has contributed much to the illustration and elaboration of Gibbs's theory of dissociation, which, though found by a different method, was nevertheless reached by a process which took for granted the general fundamental concepts of the molecular theory. The atomistic theory has not only supplied a new foundation for the equations of hydrodynamics, but it has also shown where these equations and those for the conduction of heat are in need of correction. If phenomenology deems it expedient, as it certainly must, constantly to institute new experiments for the purpose of discovering possible necessary corrections for its equations, atomism accomplishes much more in this respect, in that it enables us to point definitely to the experiments which are in most likelihood of leading to these corrections.

So also the specifically molecular theory of the ratio of the two specific heats of gases is to-day again playing an important rôle. For the simplest gases, the molecules of which behave like elastic spheres, Clausius had calculated this ratio to be $1\frac{2}{3}$, a value which applied to none of the gases then known, from which he concluded that gases so simple in structure did not exist. Where the molecules acted on impact like non-spherical elastic bodies, Maxwell found the value $1\frac{1}{3}$ for this ratio. But since the ratio for the best-known gases had the value 1.4, Maxwell likewise rejected his theory. But he had overlooked the case in which the molecules are symmetric with respect to one axis; for this case, the theory also requires for the ratio the exact value 1.4.

The old value of Clausius, $1\frac{2}{3}$, had already been obtained by Kundt and Warburg for mercury vapor, but owing to the difficulty of the experiment it had never been repeated and was almost forgotten. But the same value, $1\frac{2}{3}$, for the ratio of the two specific heats again turned up in the case of all the new gases discovered by Lord Rayleigh and Ramsay, and all the other circumstances pointed, as they had done with mercury vapor, to the extremely simple molecular structure required by the theory. What would have been the consequences for the history of the theory of

gases, if Maxwell had not committed this slight inadvertence, or if all the new gases had been known at the time of Clausius's first calculation? All the values demanded by the theory for the ratio of the two thermal capacities of the simplest gases would have been actually corroborated by experiment.

I have to mention finally the relations which obtain according to the molecular theory between the principle of entropy and the calculus of probabilities, concerning the real significance of which there may be some difference of opinion, but which, no unprejudiced person will deny, are eminently qualified to extend our intellectual horizon and to suggest new combinations both of ideas and experiments.

All these achievements, and numerous earlier acquisitions of the atomistic theory, could not possibly have been reached either by phenomenology or by energetics, and I maintain that a theory which has produced so many independent results unattainable in any other way, and for which so many other physical, chemical, and crystallographical facts speak, is deserving of cultivation and not of antagonism. But as to our conceptions of the nature of molecules, here the greatest freedom must be allowed. Thus, the theory of the ratio of the specific heats must not be abandoned because it is not universally applicable, for the molecules behave like elastic bodies only in the simplest gases, and even in these not at the highest temperatures and only with respect to their impacts. As to their more intimate, and doubtless enormously intricate, composition, we possess as yet no clues; our efforts are to be bent rather on acquiring these. The precise determination and discussion of the equations, free from all hypotheses, is equally indispensable, and is an aim to be pursued coincidently with the atomistic method; but for this reason the one is not to elevate its mathematical formalism, or the other its atomic units, to the rank of dogmas.

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To-day the battle of opinions rages tempestuously. Each holds his own to be the correct one, and it is well that he does so if his intention is but to pit its strength against that of its antagon-

ists. The rapidity of scientific advancement has strung the hopes of all to their highest pitch. What will the outcome be?

Will the old mechanics with the old forces, stripped of its metaphysical garb, continue to persist in its main features, or is it to exist henceforward merely in the pages of history, pushed from its high estate by the invisible masses of Hertz, or by some other totally different conceptions? Are the essential constituents of the present molecular theory, exclusive of all modifications and extensions, to endure for all time; is an atomistic theory totally different from the present one to reign supreme in the future; or, in subversion of the arguments that I have adduced, is the conception of a pure continuum as the most adequate representation of nature to prove victorious? Will the mechanical philosophy of nature gain the decisive victory by the discovery of some simple mechanical construct for representing the luminiferous ether; will at least mechanical models persist for all time; will new, non-mechanical models prove better adapted; will the two component factors of energy control absolutely the domain; will inquirers ultimately be content to describe each natural agent as the sum of its various component phenomena; or will theory be transformed into the mere collection of formulæ and the discussion of the equations involved?

Is it possible that the conviction will ever arise that certain representations are *per se* exempt from displacement by simpler and more comprehensive ones, that they are "true"? Or is that perhaps the best conception of the future, to imagine something of which one has absolutely no conception?

These are, indeed, interesting questions. One regrets almost that one must pass away before their decision. O arrogant mortal! Thy destiny is to exult in the contemplation of the surging conflict!

As for the rest, the wisest course is to grapple with the work at hand, and to leave off cudgelling our brains as to what the future has in store. Has the wanig century not achieved enough? An unforeseen accumulation of positive facts, a searching scrutiny and refinement of the methods of research, these are its bequest to the dawning one. A Spartan martial chorus was wont to exclaim

to the nation's youths: Be more valorous than we! It has been an ancient custom of our land to invoke blessings upon the incoming century, and we can do so in the present case most appropriately, and with no less pride than the Spartans, by wishing that it may be greater and fraught with loftier significance than the departing one. *Πατρὸς ἀμείνων.*

LUDWIG BOLTZMANN.

LEIPSIC.